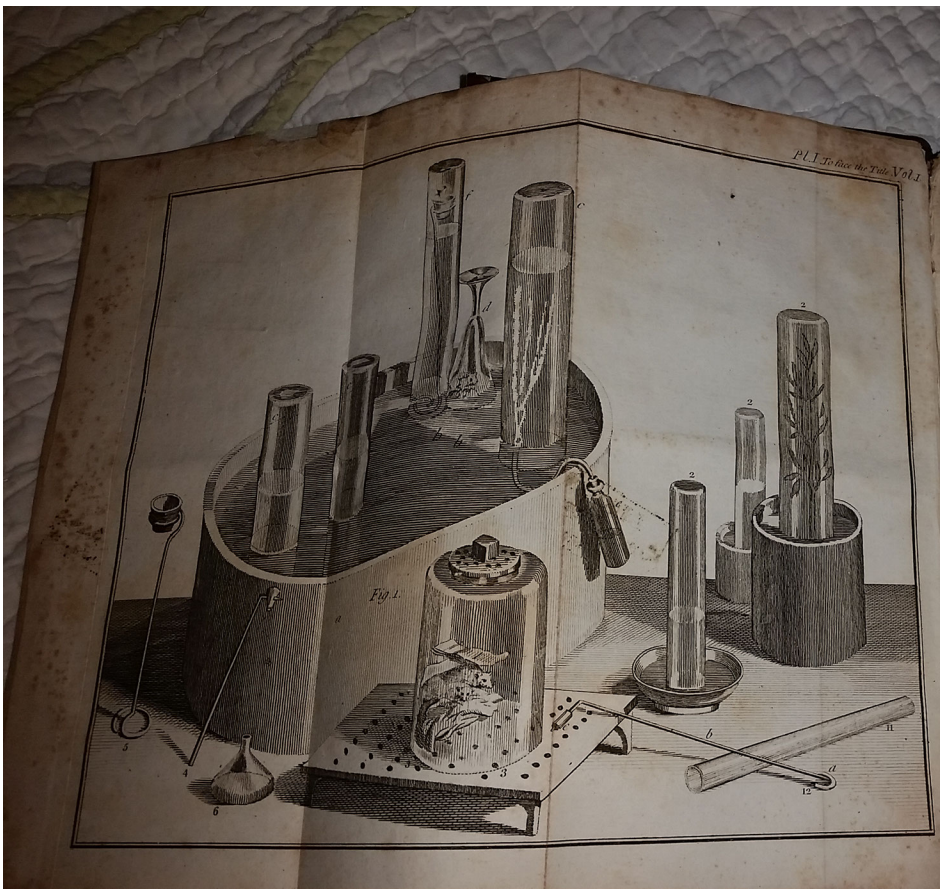

EXPERIMENTS AND OBSERVATIONS ON DIFFERENT KINDS OF *AIR*, AND OTHER BRANCHES OF NATURAL PHILOSOPHY, CONNECTED WITH THE SUBJECT.

IN THREE VOLUMES;

Being the former Six Volumes abridged and methodized, with many Additions.

By JOSEPH PRIESTLEY, LL.D. F.R.S.

AC. IMP. PETROP. R. PARIS. HOLM. TAURIN. ITAL. HARLEM. AUREL. MED. PARIS. CANTAB. AMERIC. ET PHILAD. SOCIUS.

VOL. I.

Fert animus causas tantarum expromere rerum,
Immensumque aperitur opus. LUCAN.

Motto to the First of the Six Volumes.

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8. *The electric Spark in inflammable Air.*

No kind of air, on which I have yet made the experiment, will conduct electricity; but the colour of an electric ſpark is remarkably different in ſome kinds of air, which ſeems to ſhew that they are not equally good non-conductors. In fixed air, the electric ſpark is exceedingly white; but in inflammable air it is of a purple, or red colour. Now, ſince the moſt vigorous ſparks are always the whiteſt, and, in other caſes, when the ſpark is red, there is reaſon to think that the electric matter paſſes with difficulty, and with leſs rapidity: it is poſſible that the inflammable air may contain particles which conduct electricity, though very imperfectly; and that the whiteneſs of the ſpark in the fixed air, may be owing to its meeting with no conducting particles at all. When an exploſion was made in a quantity of inflammable air, it was a little white in the center, but the edges of it were ſtill tinged with a beautiful purple. The degree of whiteneſs in this caſe was probably owing to the electric matter ruſhing with more violence in an exploſion than in a common ſpark.

9. *The Smell of inflammable Air.*

Inflammable air, when it is made by a quick proceſs, has a very ſtrong and offenſive ſmell, from

from whatever ſubſtance it be generated; but this ſmell is of three different kinds, according as the air is extracted from mineral, vegetable, or animal ſubſtances. The laſt is exceedingly fetid; and it makes no difference, whether it be extracted from a bone, or even an old and dry tooth, from ſoft muſcular fleſh, or any other part of the animal. The burning of any ſubſtance occaſions the ſame ſmell: for the groſs fume which ariſes from them, before they flame, is the inflammable air they contain, which is expelled by heat, and then readily ignited. The ſmell of inflammable air is the very ſame, as far as I am able to perceive, from whatever ſubſtance of the ſame kingdom it be extracted. Thus it makes no difference whether it be got from iron, zinc, or tin, from any kind of wood, or, as was obſerved before, from any part of an animal.

If a quantity of inflammable air be contained in a glaſs veſſel ſtanding in water, and have been generated very faſt, it will ſmell even through the water, and this water will alſo ſoon become covered with a thin film, aſſuming all the different colours. If the inflammable air have been generated from iron, this matter will appear to be a red ochre, or the earth of iron, as I have found by collecting a conſiderable quantity of it; and if it have

have been generated from zinc, it is a whitiſh ſubſtance, which I ſuppoſe to be the calx of the metal. It likewiſe ſettles to the bottom of the veſſel, and when the water is ſtirred, it has very much the appearance of wool. When water is once impregnated in this manner, it will continue to yield this ſcum for a conſiderable time after the air is removed from it. This I have often obſerved with reſpect to iron.

SECTION II.

Inflammable Air decompoſed by Heat, in Tubes of Flint Glaſs.

THIS kind of air remains unchanged when it is expoſed to heat in a tall jar of flint glaſs, in which it had free liberty to expand. I made this experiment at the ſame time with the ſimilar one that I ſhall have occaſion to mention on nitrous air. This air, as well as the nitrous, recovered its former dimenſions when it was cold, and appeared to be unchanged in its quality.

A very

A very ſingular decompoſition of inflammable air I obſerved in conſequence of expoſing a great variety of ſubſtances to the influence of a ſand heat, which I kept up for ſeveral months. Among other things, I buried in this hot ſand glaſs tubes hermetically ſealed, and previouſly filled with all the different kinds of air. I filled them in the following manner.

Having provided myſelf with glaſs tubes about four feet long, and about one third or one half of an inch in diameter, and of ſuch a thickneſs as that I could eaſily melt them with the flame of a couple of candles and a common blow pipe, I firſt ſealed the tubes at one end, then filled them with quickſilver, and placed them inverted in a baſon of the ſame. After this, either transferring the air in a bladder, from the jars in which they had been ſtanding in water, or generating the air a-freſh, if it was of a kind not to bear the contact of water, I filled the tubes completely with the kinds of air on which I wiſhed to make the experiment, diſplacing the quickſilver. This being done, I inclined the tube, and applying the flame of my candles with ſome care (holding the blow pipe in my mouth only, and keeping firm hold of the tube on each ſide of the place to which I was applying the heat) I melted the glaſs, and took off what lengths of it I pleaſed; and

every

every piece was, of course, hermetically sealed. These pieces I marked with a file, keeping an account of the meaning of the marks, that when I took them out of the sand, I might presently know with what kind of air they had been filled.

When I was performing this part of the process with inflammable air in flint glass tubes, I observed that the places to which I applied the heat were generally tinged black; but I gave little attention to this circumstance, thinking it might be something accidental; and without any particular expectation, I buried these tubes in the sand, together with the others. This was on the 25th of September, 1777.

On the 20th of January following, I examined these tubes, together with every thing else that had been exposed to the same heat. The tube containing the inflammable air was ten inches long, and by some accident was broken; but it was jet black throughout. At this I was very much surprized, but I did not then suspect that it was at all owing to the inflammable air with which it had been filled; thinking it might have been occasioned by some phlogistic matter in the sand, or in some of the vessels that had burst in its neighbourhood.

Reflecting, however, on this odd circumstance, and thinking, from the uniformity of the tinge, that,

that, *possibly*, it might have been occasioned by the inflammable air, I filled another small glass tube with the same air; and, sealing it hermetically, buried it deep in sand, contained in an iron pot, which I set on the fire, and made very hot, nearly red; and taking it out the next day, I found the tube quite black, except a small part on one side of that end which had been uppermost, about two inches higher than the other, and which, consequently, had not been exposed to so great a degree of heat.

Being now fully satisfied that the blackness of the tube was *certainly* occasioned by heating the inflammable air within it, in circumstances in which it could not expand, I proceeded to examine the state of the air, and frequently found it to be inflammable; but, in general, the quantity was too small to make a satisfactory experiment.

Putting two glass tubes, about four inches in length, and a quarter of an inch in diameter, into a sand furnace, I kept them in it two days; when I took them out, and observed that the tube which I had placed at the bottom of the sand, in the greatest degree of heat, was nearly melted, and perfectly *blue*, like indigo; while the other tube, which had not been exposed to so great a degree of heat, was of a beautiful jet black throughout.

At one time I had a suspicion that this blackness communicated to the glass was something precipitated

tated from the iron, by the ſolution of which the inflammable air had been made; but I was ſoon convinced of the contrary, by finding that the effect was the very ſame when the inflammable air was made from *zinc*.

I ſoon found that there was no occaſion for ſo long a proceſs to produce this effect, at leaſt upon the glaſs. For it begun to be diſcoloured the moment it was red hot, or rather when it became ſoft; as was evident by holding one of the tubes in an open fire, or in the flame of a candle. For wherever the heat was applied, the blackneſs immediately took place, without affecting any other part of the tube.

When I examined this black tinge narrowly, I found that it did not penetrate the glaſs, but formed a delicate ſuperficial tinge, leaving the glaſs as perfectly poliſhed as before the proceſs. But the blackneſs was indelible; at leaſt, it could not be ſcraped off without tearing the ſurface of the glaſs, and it made no change in it with reſpect to electricity. For the tube thus blackened was as perfect a non-conductor as ever.

The blue colour of the glaſs that was moſt heated, Mr. Delaval informed me, was owing to ſomething of *iron* in the compoſition of the glaſs. That it alſo depended upon the *degree of heat*, I aſcertained by placing one of theſe tubes in a vertical poſition in the ſand

ſand heat. For the lower end of the tube, which was moſt heated, had acquired a deep blue colour, and it paſſed into the black at the upper end of the tube without any intermediate colour. There was alſo no other colour higher than the black; ſo that the firſt tinge that the glaſs receives is a perfect black. Yet viewing the firſt tinge that it receives by the light of a candle placed beyond it, it ſeemed to have a ſhade of *red*.

As I was ſenſible that the blackneſs was owing to the precipitation of *phlogiſton* from the inflammable air, I thought it poſſible that ſome ſubſtance which had a near affinity with phlogiſton might diſcharge it; and trying *minium*, it ſucceeded immediately. Having filled one of theſe black tubes with this metallic calx, the moment I made it red hot, the blackneſs intirely diſappeared, and left the tube as tranſparent as ever it had been.

In the firſt experiment of this kind I uſed minium, out of which all its air had been expelled by heat, and which is of a yellow colour. In this proceſs it became whiter, and adhered a little to the glaſs. When I ſcraped it off, I could not be quite ſure that any part of it was become real *lead*; but it evidently approached towards a metallic ſtate, by being of a more compact texture than before.

In this ſtate of the experiments I communicated the reſult of my obſervations to my friend Mr. Bewly, who ſuggeſted to me, that, probably, it was

the

the *lead* in the glaſs tubes that had attracted the phlogiſton; and I preſently found this to be the caſe. For when I had filled a *green glaſs* tube with the inflammable air, and ſealed it hermetically, as I had done the flint glaſs tubes, I expoſed it to a melting heat, which is greater than that which flint glaſs will bear, without producing any change of colour in it. What remained of the air in the tube, that did not eſcape when part of it was melted, was ſtill ſtrongly inflammable.

It appears, therefore, from this experiment, that the calx of lead, in the form of glaſs, has a ſtronger affinity with phlogiſton than any thing in the compoſition of inflammable air, in a degree of heat capable of melting glaſs. Or, if there be no proper conſtituent part of inflammable air beſides phlogiſton, the attraction of the calx is ſo great, as to reduce the phlogiſton from an elaſtic and uncombined ſtate to a fixed and combined one.

Having, by means of theſe glaſs tubes, effected a complete decompoſition of inflammable air, the phlogiſton in it having united with the glaſs of the lead; I thought that, if there had been any *acid* in its compoſition, it would then be diſengaged, and be found in the tube. In order to find whether there was any acid in it, or not, I poured into one of theſe tubes a ſmall quantity of water made blue with the juice of turnſole; but it came out as blue as it went in.

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SECTION III.

Of ſulphurated inflammable Air.

THERE is no kind of air which admits ſuch a variety of modifications as the inflammable; nor ſhall we think this extraordinary, when we conſider that phlogiſton, which is the diſtinguiſhing ingredient in it, enters into a greater variety of combinations with *ſolid ſubſtances* than perhaps any other principle in nature, and is the cauſe of a greater variety of properties in them. Spirit of wine, oil, ſulphur, charcoal, and metals, are ſubſtances as different from each other, both in their external appearance, their degrees of conſiſtence, and other chemical properties, as any things in nature, and yet the principal ingredient in them all is the ſame phlogiſton, as may be proved by the actual transferring of it from any one to any other of them. Inflammable air likewiſe extracted from each of theſe ſubſtances, as alſo that from putrid vegetables, and by other proceſſes, of which an account has been given in the preceding ſections, are all remarkably different, and appear to be ſo, as we ſhall preſently ſee, when they are decompoſed. I ſhall now give an account

it does not burn blue on a hot iron, yet shews evident signs of containing sulphur. For when the nitrous acid has taken from it its superfluous phlogiston, it has both the colour and the smell of sulphur.

SECTION IV.

Metals, and other Substances containing Phlogiston, formed by imbibing inflammable Air.

THERE are few subjects, perhaps none, that have occasioned more perplexity to chemists, than that of *phlogiston*, or, as it is sometimes called, *the principle of inflammability*. It was the great discovery of Stahl, that this principle, whatever it be, is transferable from one substance to another, how different soever in their other properties, such as sulphur, wood, and all the metals, and therefore is the same thing in them all. But what has given an air of mystery to this subject, has been that it was imagined, that this principle, or substance, could not be exhibited except in combination with other substances, and could not be

made to assume separately either a fluid or solid form. It was also asserted by some, that phlogiston was so far from adding to the weight of bodies, that the addition of it made them really lighter than they were before; on which account they chose to call it *the principle of levity*. This opinion had great patrons.

Of late it has been the opinion of many celebrated chemists, Mr. Lavoisier among others, that the whole doctrine of phlogiston is founded on mistake, and that in all cases in which it was thought that bodies parted with the principle of phlogiston, they in fact lost nothing, but on the contrary acquired something; and in most cases an addition of some kind of air; that a *metal*, for instance, was not a combination of two things, viz. an *earth* and *phlogiston*, but was probably a simple substance in its metallic state; and that the calx is produced not by the loss of phlogiston, or of any thing else, but by the acquisition of air.

The arguments in favour of this opinion, especially those which are drawn from the experiments that Mr. Lavoisier made on mercury, are so specious, that I own I was myself much inclined to adopt it. My friend Mr. Kirwan, indeed, always held that phlogiston was the same thing with inflammable air. I did not, however, accede

to

to it till I thought I had discovered it by direct experiments, made with general and indeterminate views, in order to ascertain something concerning a subject which had given myself and others so much trouble.

I began with repeating the experiments in which I had found that inflammable air, made red-hot in flint glass tubes, gave them a black tinge, and was in a great measure absorbed, which I discovered to be owing to the calx of lead in the glass, attracting phlogiston from the inflammable air.

I found, however, great difficulty in repeating these experiments; and the quantity of inflammable air operated upon in them, is necessarily so small, that the result is always liable to much uncertainty. I thought, therefore, that throwing the focus of a burning lens upon a quantity of pounded flint glass, surrounded with inflammable air, or rather on the calx of lead alone, in the same circumstances, would be a much easier experiment, and might bring me nearer to my object; and on making the experiment it immediately answered far beyond my expectation.

For this purpose, I put upon a piece of a broken crucible (which could yield no air) a quantity of minium, out of which all air had been extracted; and placing it upon a convenient stand, intro-

introduced it into a large receiver, filled with inflammable air, confined by water. As soon as the minium was dry, by means of the heat thrown upon it, I observed that it became black, and then ran in the form of perfect lead, at the same time that the air diminished at a great rate, the water ascending within the receiver. I viewed this process with the most eager and pleasing expectation of the result, having at that time no fixed opinion on the subject; and therefore I could not tell, except by actual trial, whether the air was decomposing in the process, so that some other kind of air would be left, or whether it would be absorbed *in toto*. The former I thought the more probable, as, if there was any such thing as phlogiston, inflammable air, I imagined, consisted of it, and something else. However, I was then satisfied that it would be in my power to determine, in a very satisfactory manner, whether the phlogiston in inflammable air had any *base* or not, and if it had, what that base was. For seeing the metal to be actually revived, and that in a considerable quantity, at the same time that the air was diminished, I could not doubt, but that the calx was actually imbibing something from the air; and from its effects in making the calx into metal, it could be no other than that to which

which chemists had unanimously given the name of *phlogiston*.

Before this first experiment was concluded, I perceived, that if the phlogiston in inflammable air had any base, it must be very inconsiderable: for the process went on till there was no more room to operate without endangering the receiver: and examining, with much anxiety, the air that remained, I found that it could not be distinguished from that in which I began the experiment, which was air extracted from iron by oil of vitriol. I was, therefore, pretty well satisfied that this inflammable air could not contain any thing besides phlogiston; for at that time I reduced about forty five ounce measures of the air to five.

In order to ascertain a fact of such importance with the greatest care, I afterwards carefully expelled from a quantity of minium all the phlogiston, and every thing else that could have assumed the form of air, by giving it a red heat when mixed with spirit of nitre; and immediately using it in the manner mentioned above, I reduced a hundred and one ounce measures of inflammable air to *two*. To judge of its degree of inflammability, I presented the flame of a small candle to the mouth of a phial filled with it, and observed, that it made thirteen separate explosions, though weak ones (stopping the mouth of the

the phial with my finger after each explosion) when fresh made inflammable air, in the same circumstances, made only fourteen explosions, though stronger ones.

After this experiment I could not hesitate to conclude*, that this inflammable air went totally, and without decomposition, into the lead which I formed at that time; and if the necessary circumstances of the experiment be considered, it will be thought extraordinary that, even admitting this, the result should be so decisively clear in favour of it: for, in the first place, the greatest care must be used to expel all air from the minium, and it must be used before it can have attracted any from the atmosphere; and in the next place, the water also (a considerable quantity of which must be used, and which will also be heated in the process) should be made as free from air as possible. In these circumstances, had I found the small residuum, of two ounce measures from a hundred and one, to have been phlogisticated or fixed air, I should not have been disappointed; and it would not have prevented my concluding

* In this conclusion, I overlooked one obvious consideration, viz. that water, or any thing soluble in water, might be the basis of inflammable air. All that could be absolutely inferred from the experiment was, that this basis could not be any thing that was capable of subsisting in the form of *air*. It will be seen, that I afterwards made the experiment with the air confined by mercury.

ing that *phlogiſton* was the ſame thing with *inflammable air*, contained in a combined ſtate in metals, juſt as fixed air is contained in chalk and other calcareous ſubſtances; both being equally capable of being expelled again in the form of air.

Afterwards, uſing a calx of lead, which had been prepared in the ſame manner with the former, but which had remained for ſome weeks expoſed to the air, I found, that when by uſing it I had reduced 150 ounce meaſures of inflammable air to ten, this reſiduum was phlogiſticated air. But examining this calx ſeparately, I found that it gave, by heat in a glaſs veſſel, a conſiderable quantity of phlogiſticated air.

I muſt obſerve, that the minium ſhould not be reduced to a perfectly compact *glaſs of lead*; for then it will be too refractory to be eaſily revived by this proceſs. Making uſe of ſome of it, I found that I could only melt it; but that a copious black fume came from it, and coated the inſide of the receiver: an experiment which I ſhall repeat and conſider. I muſt alſo obſerve, that the lead which I procured in the above mentioned proceſs was not to be diſtinguiſhed from any other lead, and that the inflammable air was all procured from iron by oil of vitriol.

When I made uſe of inflammable air from wood, I found, that though I was able to reduce minium with

with it, it was effected with more time and difficulty. Forty ounce meaſures of this kind of inflammable air I reduced to twenty five; after which I found that the heat of the lens produced only *glaſs of lead*, and *no metal*. The air was ſtill, however, inflammable; and there was a ſmall mixture of fixed air in it. This kind of inflammable air, which burns with a lambent flame, I have ſome reaſon to think, conſiſts of an intimate union of fixed air with that which is of the *exploſive* kind extracted from metals. The reſult of thoſe experiments which I made with that kind of inflammable air which is collected in the proceſs for making phoſphorus, and which burns with a lambent yellow flame, was ſimilar to thoſe which I made with inflammable air from wood, which burns with a lambent white flame.

Having had this remarkable reſult with inflammable air, I immediately tried all the other kinds of air in the ſame manner; but in none of them did I procure any thing from the minium beſides glaſs of lead, except in alkaline air, and vitriolic acid air. In fixed air, nitrous air, phlogiſticated air, marine acid air, fluor acid air, as alſo in common and dephlogiſticated air, I got no *metal* at all. In vitriolic acid air there was but a ſmall quantity of lead produced, and I have obſerved that this kind of air imparts a certain portion of phlogiſton to common air (or rather imbibes

bibes a part of the dephlogisticated air from it) rendering the remainder in some measure phlogisticated, though by no means in so great a degree as nitrous air.

Though nitrous air and phlogisticated air certainly contain phlogiston, they appear by these experiments to hold it too obstinately to part with it to minium in this process, notwithstanding nitrous air quits it so readily to respirable air. I would observe, that there were some peculiar appearances in the experiments I made to revive the calx of lead in these kinds of air in which the attempt did not succeed; but I must repeat the experiments, and note the appearances more accurately, before I report them.

In alkaline air lead seems to be formed from the minium as readily as in inflammable air, and indeed I thought rather more so; and this is a remarkable confirmation and illustration of those experiments, in which, by taking the electric spark in a quantity of alkaline air, I converted it into three times as much pure inflammable air; an experiment which, on account of the extraordinary nature of it, I have repeated many times since I first published the account of it, and always with the same result.

This experiment also throws some light upon those in which, by exposing iron to nitrous air, I pro-

duced a strong smell of volatile alkali; an experiment which I have also frequently repeated with the same result. The reviving of lead in alkaline air may also help us to conceive how all *acids* should have an affinity both to *phlogiston* and to *alkalies*, which have hitherto appeared to be things so very different from each other; since, from these experiments, it is probable that one of them is some modification of the other, or a combination of something else with the other. To trace the connexion between the alkaline and inflammable principles, is a curious subject; and from these hints it may, perhaps, not be very difficult to prosecute it to advantage. It is evident, however, from the following experiments, that alkaline air is the compound, and inflammable air, or phlogiston, the more simple substance of the two.

From five ounce measures and a half of alkaline air I got, by means of litharge, seventeen grains of lead, besides some that was dissolved in the mercury, by which the air was confined. There remained two ounce measures and a half, which appeared to be phlogisticated air, and to have no fixed air in it. At another time, in eight ounce measures of alkaline air I got fifteen grains of lead, besides what was dissolved in the mercury, which seemed to be a good deal in proportion to it.

inflammable air may be expelled from perfect iron in this very process*.

Silver I evidently revived from a solution of it in spirit of nitre precipitated by salt of tartar, and also from *luna cornea*. A quantity of this last substance absorbed twenty three ounce measures of inflammable air; but I could not get any calx of silver free from small grains of the perfect metal, which was easily discovered by a magnifier, and therefore I could not ascertain the quantity of inflammable air absorbed by it.

Small grains of regulus of *cobalt* I produced from zaffre, and inflammable air was absorbed; but I did not estimate the quantity.

A quantity of *manganese* absorbed seven ounce measures of inflammable air; but I could not perceive any thing in it which had the appearance of metal. But I imagined I had not heat enough for the purpose; and mixing with it some calcined borax, I repeated the experiment, when there was again an evident absorption of air, and in the course of that experiment, I once thought that I did perceive a small globule of metal.

Zinc and *arsenic* were only sublimed in this process. The same was the case with the glass of

* I have since found that inflammable air cannot be expelled from iron by heat, without some moisture, which therefore seems necessary to its constitution.

antimony;

antimony; but the experiment was attended with this peculiar circumstance, that when the glass was melted in inflammable air, it formed itself into needle-like crystals, arranged in a very curious manner; and I could not produce that appearance in other kinds of air.

Inflammable air being clearly imbibed by the calces of metals, and thereby reviving them, is a sufficient proof of its *containing* what has been called phlogiston; and its being absorbed by them *in toto*, without decomposition, is a proof that, exclusive of water, it is nothing besides *phlogiston in the form of air*, unless there should be something solid deposited from it at the same time that the proper phlogistic part of it is absorbed. With respect to this, I can only say that, in the course of the experiments, I did not perceive any thing of the kind; for though in some of the processes there was a black smoke produced, in others I could perceive nothing but part of the calx subliming, and clouding the glass. On this account, however, I could not pretend to ascertain the weight of the inflammable air in the calx, so as to prove that it had acquired an addition of weight by being metalized, which I often attempted. But were it possible to procure a perfect calx, no part of which should be sublimed and dispersed, by the heat necessary to be made use of in the process, I

should not doubt but that the quantity of inflammable air imbibed by it would sufficiently add to its weight.

Beſides the formation of metals from their calces, I had other proofs, and of a nature ſufficiently curious, of inflammable air containing phlogiſton. Thus, by means of it, I was able to make *phoſphorus*, *nitrous air*, *liver of ſulphur*, and *ſulphur* itſelf, in all of which phlogiſton is acknowledged to be a principal ingredient.

Throwing the focus of the lens upon a quantity of that glaſſy matter which is made from calcined bones by oil of vitriol in inflammable air, ſome of it was abſorbed, and all the inſide of the receiver was covered with an orange coloured ſubſtance, which had a ſtrong ſmell of phoſphorus. I then wanted ſun-ſhine to continue the experiment; but I was ſatisfied that there was ſufficient proof of phoſphorus being actually formed in this manner. With alkaline air I ſucceeded much better.

In two ounce meaſures and a half of this air, I produced, from the glaſſy matter mentioned above, two grains of phoſphorus in one maſs, the veſſel being only filled with white fumes during the proceſs. One fourth of the bulk of the air remained, and this was inflammable, burning with a yellow lambent flame, exactly like that which is produced in the proceſs for making phoſphorus.

That nitrous air contains phlogiſton is ſufficiently evident, if there be any ſuch thing as phlogiſton: and I have farther proved, that it contains very nearly as much phlogiſton, in proportion to its bulk, as inflammable air itſelf. I had now, however, the farther ſatisfaction to be able to make nitrous air from its two conſtituent principles, viz. nitrous vapour and inflammable air. The moſt eaſy proceſs for this purpoſe is, to throw a ſtream of nitrous vapour into a large phial previouſly filled with inflammable air. In this manner nitrous air is inſtantly formed, and in great quantities; but as this nitrous vapour is produced by the rapid ſolution of biſmuth in ſpirit of nitre, which at the ſame time produces a quantity of nitrous air, the experiment is not quite unexceptionable. I therefore attempted the ſame thing in the following manner.

Taking a quantity of what I have called a *nitrated calx* of lead, which I firſt produced by uniting nitrous vapour to minium (in conſequence of which, from being a red and powdery ſubſtance, it becomes white, compact, and brittle) I placed it upon a ſtand, in a receiver filled with inflammable air, and throwing the focus of the lens upon it, there was a diminution of the inflammable air, which amounted to about two thirds of the whole, and during this time lead was revived from the

calx. After this there was no more diminution of the air, or revival of the calx: and then examining what remained of the air, I found it to be all ſtrongly nitrous: and, from the circumſtances in which it was produced, it muſt have been formed from the nitrous vapour contained in the calx, and the inflammable air in the receiver. In order to aſcertain the purity of this nitrous air, I mixed it with an equal quantity of common air, and found that they occupied the ſpace of 1.32 meaſures. Freſh nitrous air made in the uſual way, and mixed with common air in the ſame proportion, occupied the ſpace of 1.26. This difference aroſe not from any impurity in the nitrous air, but from the mixture of the dephlogiſticated air, which is alſo expelled from this calx by heat.

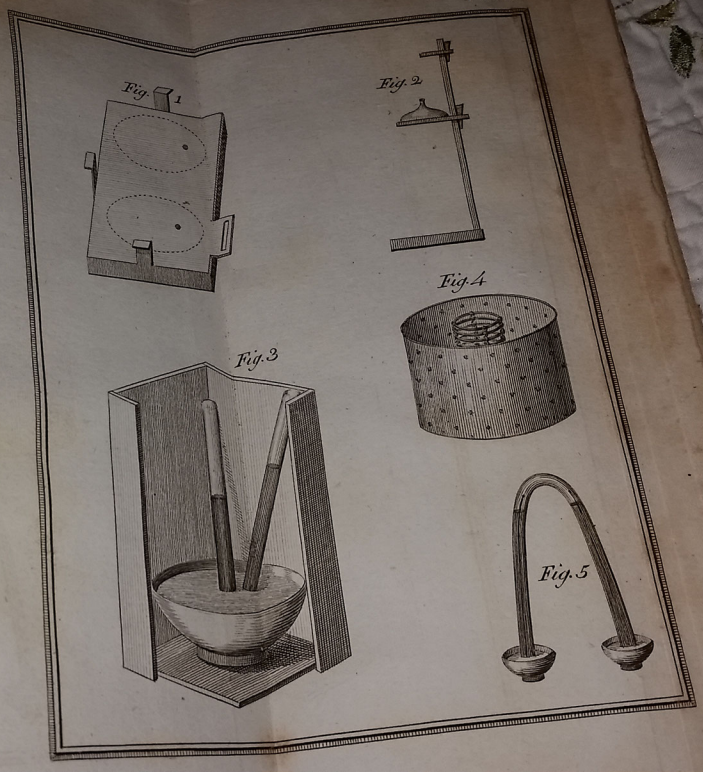
Liver of ſulphur was procured by throwing the focus of the lens upon vitriolated tartar in inflammable air, and it appeared to be perfectly well formed.

Laſtly, to produce *ſulphur*, I threw the focus of the lens on a quantity of oil of vitriol, contained in an hollow earthen veſſel, and evaporated it to dryneſs in a receiver filled with inflammable air; in conſequence of which the inſide of the receiver acquired a whitiſh incruſtation, which when warmed had a ſtrong ſmell of ſulphur; and repeating

repeating the proceſs in the ſame receiver, I was able, this ſecond time, to ſcrape off enough of the matter to put on a piece of hot iron, and to produce the genuine blue flame, as well as the peculiar ſmell, of ſulphur.

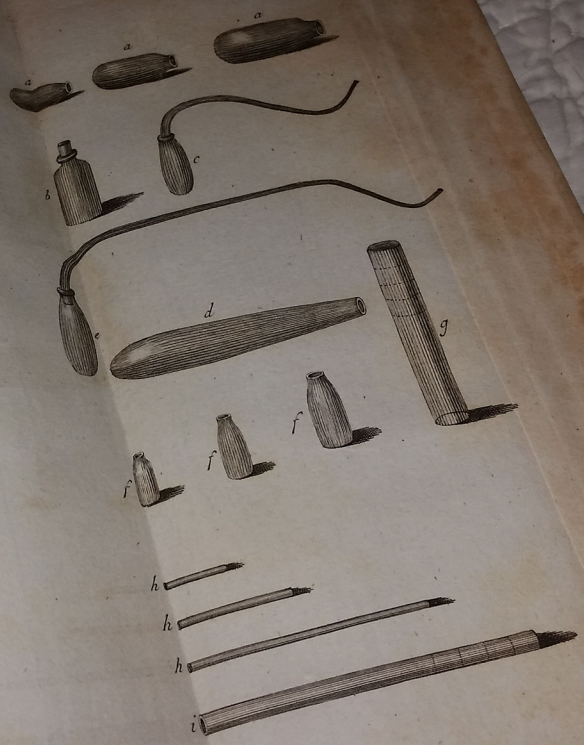
PART

Pl. III.



discharg-
rent," devised by
exemplify the above statement.
short pulses, separated by relatively enormous inte-
cally represented, if the distance on the "*time-line*," or "ab-
scissa," corresponding to the rise and fall of a *single current*
impulse be *one inch*, then, the distance to the *next impulse*, as
represented on the Abscissa, would be *seventeen and one-half*
miles!

(I think I was lying
when I wrote this
7.7.5)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Pl. V

Fig. 1.

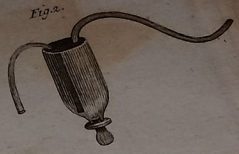


Fig. 2.

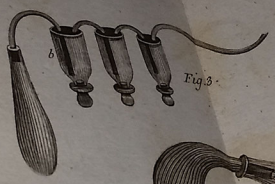
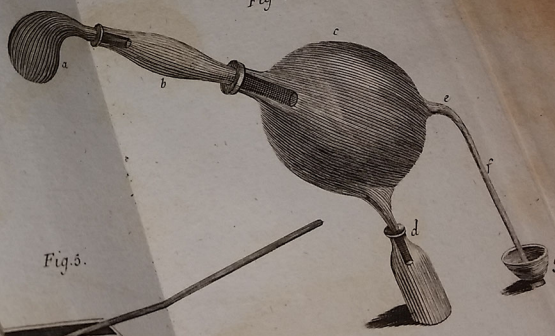


Fig. 3.

Fig. 4.



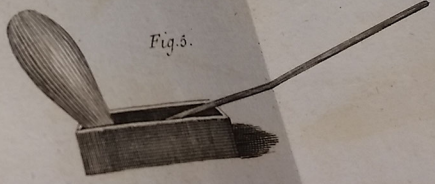


Fig. 5.

Pl. VI.

Fig. 1.

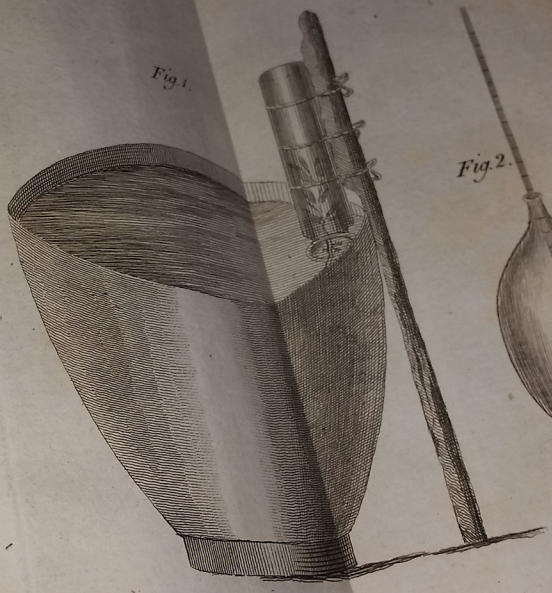
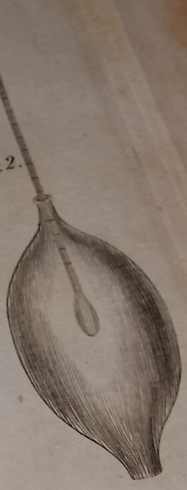


Fig. 2.




Pl. VII.
Fig. 1
Fig. 5
Fig. 4
Fig. 3
Fig. 6
Fig. 2

Pl. VIII